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April 6, 1976

Mr. John J. Marshall  
Barrister and Solicitor  
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CALGARY, Alberta  
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Dear Mr. Marshall:

We have reviewed Dr. Geist's report and have found several erroneous references to our reports which we feel seriously question the validity of his criticism. Examples of the inaccuracies are outlined below.

Dr. Geist attacks our choice of research topics stating on page 24 of his report that *"There is no evidence in the reports examined that the consultants systematically contrasted stimuli and situations aversive and damaging to caribou against the stimuli and situations inadvertently generated by construction and operating phases, as well as secondary supportive activities, of the pipeline"*, and on page 25, he states *"...topics for investigation by the applicant appear to have been chosen on a random ad hoc basis,..."*

The approach to our mammal studies is outlined on pages 30-33 of Arctic Gas' Environmental Statement. The portion of the discussion pertaining to disturbance studies states *"Evaluation of baseline information in the context of a knowledge of the fundamental aspects of construction and operation of the pipeline enabled identification of potential impacts. Where those could not be avoided through recommendations on location and scheduling, experimental studies were designed to determine the significance of potential impacts and to recommend modification in design."*

*"One category of potential impacts which warranted experimental studies was that of mechanical disturbance. Three types of mechanical disturbances most likely to be created by pipeline activities are:*

- i) noise from gas compressor stations*
- ii) noise and motion of aircraft*
- iii) noise and human presence associated with construction equipment."*

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*"Although the latter will only occur during the construction period and problems can be largely avoided through careful scheduling, the first two types of disturbance are unavoidable since they will continue throughout the operation period. As a result, studies were designed to test the response of certain mammals to simulated noise of compressor stations and to aircraft disturbance."*

Obviously the topics of research were not chosen randomly.

Dr. Geist states that our research on aircraft harassment of large mammals was faulty in methodology, but here again, this contention is supported by inaccurate references to our reports.

On page 25, point 1, Geist states *"The method of recording disturbance to caribou is based on gross behavioral indicators, i.e. No Reaction, Mild Reaction and Strong Reaction."* The fact is that responses were recorded in detail and were later classified into three categories. This is clearly stated in the reports. In *McCourt et al.* (1974) on page 184 it states *"The degree of reaction to the aircraft was classified in three categories"*, and in *McCourt and Horstman* (1974) on page 3 it states *"Responses were described by the observer and later translated into the categories 'nil', 'moderate', and 'strong'."*

On page 26 of Geist's report it states *"This description and establishment of response categories in Klein (1973) and DeBock and Surrendi (1974) are better"* and on page 27, Geist makes reference to *"Klein (1973, Table 2) using a somewhat more precise system of classifying responses of caribou to aircraft..."* However, in Table 2 of Klein's paper we see that in a manner similar to ours, Klein grouped responses into three categories - nil, mild, and strong. Since the copy of the draft report of DeBock and Surrendi contains no tables and figures, it is impossible to determine how their data were handled.

On page 26, point 3, of Geist's report, it states *"It is not clear how a decision was reached that caribou were responding at the level of a given response category"* and goes on to say *"Did the researchers count individual caribou according to various response categories? Did they evaluate an overall response category for the whole herd? We do not know."* Yet in *McCourt and Horstman* (1974) on page 3, it states that *"Response data refer to the response of the majority of the group."*



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On page 26, point 3, of Geist's report he states *"The response categories were not subject to validity checks for consistency between observers, years and studies, since McCourt and Horstman (1973) lump data from two studies - theirs and Roseneau's. We do not know therefore what was recorded."* And similarly on page 28 in his point 5, Geist states *"Later the 'data' so gathered was lumped with that of Roseneau et al. (1974), without any evidence given that the manner of gathering data by these authors was comparable."* Yet on page 3 of McCourt and Horstman (1974) it states *"Methods for the collection of the 1972 data in the Yukon are described in McCourt et al. (1974a). Similar methods were used in gathering the 1973 Yukon and Alaska data."* The "Alaska data" obviously refers to Roseneau's.

Beginning on page 26 and continuing on page 27 of Geist's report, he states that *"The results of studies based on such inadequate methodologies cannot be adequately duplicated. That this is so is suggested by a comparison of the data of McCourt et al. (1974) and Klein (1973). Thus in tables 38 and 40 of McCourt et al. (1974), we note that at 1000 foot elevation, fixed-wing aircraft still precipitated some 18 per cent mild plus strong responses and, depending on group size, an even higher incidence of disturbance. (On this latter point the data is weak due to small sample size). Klein (1973, Table 2), using a somewhat more precise system of classifying responses of caribou to aircraft, found the number of mild and strong responses of caribou to aircraft to be 62 per cent at elevations exceeding 500 feet. The above example may only serve to illustrate how different observers evaluate responses differently."*

The obvious question is what value does this comparison have when data from McCourt et al. are used for elevations exceeding 1000 feet and data from Klein are used for elevations exceeding 500 feet. Data in McCourt et al. were grouped under <300', 300' to 600', 600' to 1000' and >1000', while Klein's were grouped under <200', 200' to 500' and >500'. Thus a direct comparison cannot be made. It is also interesting that Geist notes that our sample size for the greater than 1000 feet category is small but says nothing about Klein's sample size. In fact, while Klein's sample size for >500 feet was only 16 observations (which is definitely too small for any conclusions), ours for >1000 feet (to which Geist refers) was 113 and for >500 feet would have exceeded 365 observations (the sum of our sample size for >1000 feet and 600 to 1000 feet).





In several cases, Geist compares our results with those of Klein (1973) and DeBock and Surrendi (1974) and implies that our results are less credible. We feel that it is significant that the sample size in Klein's study was 75 observations, the sample size in DeBock and Surrendi's study was 454 (from page 63) while the sample sizes in McCourt *et al.* (1974) and McCourt and Horstman (1974) were 859 and 4134 respectively.

On page 30 of his report, Geist states "*The literature survey of the consultants is inadequate, in that papers pertinent to harassment of ungulates were not mentioned or the content of cited papers was not comprehended.*" Regarding the literature survey, Geist has failed to recognize the distinction between a report based on a literature review (exemplified by the first portion of his report) in which all literature reviewed is cited and a report based on new data in which a comprehensive review of the literature is undertaken but only that which is relevant to the discussion of the data is cited. Regarding his statement that "*the content of cited papers was not comprehended*", the foregoing examples of inaccurate references to our reports by Geist fall in the same category.

Geist states further that the consultants failed to notice that large mammals could be habituated to man-made environments. In fact, however, a discussion of habituation appears on page 26 and 28 of McCourt and Horstman (1974).

Also on page 30, Geist states that "*Even if the data presented could be accepted as valid, the recommendations that an overflight ceiling of 1000 feet be established is unacceptable as determined from Tables 28, 38 and 40 of McCourt et al. (1974). The "mild", plus "strong" responses of caribou are still 14.3 - 18 per cent. Thus about one flight in five at 1000 feet elevation or higher will precipitate a response by adult caribou that according to the consultants entails running.*" If Geist had referred to the description of response categories on page 184 and 185 of McCourt *et al.* (1974) he would have found that only the "strong" response category entails running. Therefore using the example he took from Table 38 in which a total of almost 18 per cent of the responses at >1000' were "mild" or "strong", the fact is that only 0.9 per cent were "strong" (entailing running).

Geist's report regarding our studies on the response of caribou to cutlines is also characterized by misleading statements. On page 32 he states that "*The consultants did not evaluate the effects of seismic lines on caribou, and presented data too inadequate to assess this impact.*" Unfortunately Dr. Geist misinterpreted the purpose of





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the study. On page 136 of McCourt *et al.* (1974) it states that "*...the study was undertaken to estimate the probability of occurrences, and the likely extent of a deflection along the proposed pipeline corridor.*"

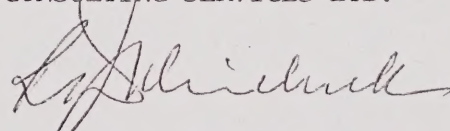
On page 33 Geist makes reference to Table 37 on page 165 of McCourt *et al.* (1974) and states that "*...only about one-third of the deflections of 30-45° were followed.*" The fact is that in only 18 per cent of the cases of encounters oriented in the range of 30-45° was the corridor followed.

Geist presents calculations to show what deflections mean in terms of energy cost to caribou. His calculations of the cost of deflections are based on a total cost per day of a migratory caribou weighing 77.7 kg of about 3100 cal (presumably he meant 3100 kcal). White *et al.* (1975) estimated the daily maintenance energy requirement of grazing caribou at 190 kcal/d per kg<sup>0.75</sup>. For a 77.7 kg caribou, daily maintenance energy requirement would be 4978 kcal per day. For a migrating caribou, the energy requirement per day would be considerably higher. Using these calculations would reduce by approximately one-half the percentage additional daily cost of deflections calculated by Geist. Thus, the cost of traveling an additional 0.40 miles as a result of a deflection may amount to approximately 0.3 per cent of the total daily energy requirement. However, when one considers that caribou are probably attracted to seismic lines because of the ease of travel along them, this factor may well compensate for the energy cost of the small increase in distance.

While there are several other points where we would disagree with Dr. Geist, most are matters of professional opinion. We trust that the exposure of the erroneous and misleading references to our work will at least set the record straight to enable an objective evaluation of our research and the validity of Dr. Geist's criticisms.

Sincerely yours,

RENEWABLE RESOURCES  
CONSULTING SERVICES LTD.



R.D. Jakimchuk  
President

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